



Центр "Петербургское Востоковедение"
St.Petersburg Centre for Oriental Studies

ПЕТЕРБУРГСКОЕ ВОСТОКОВЕДЕНИЕ

St.Petersburg Journal of Oriental Studies

**выпуск 6
volume 6**

Центр
"Петербургское Востоковедение"

Санкт-Петербург
1994

C. Renfrew. *Archaeology and Language. The Puzzle of Indo-European Origins*. London, 1987.

On the whole, I have no quarrel with Renfrew's general approach to the problem, especially with such chapters as 1. The I.-E. Problem in Outline; 2. Archeology and the Indo-Europeans; 6. Language, Population and Social Organization — especially there is no quarrel with the latter chapter which brilliantly shows the connection between agriculture and enormous population growth, and of the latter with language displacement; I also agree with chapter 10. Indo-European Mythologies.

However, Renfrew's erudition in linguistics is insufficient, and in this field his statements tend to be wrong or, at least, unreliable (his chapter 3. Lost Languages and Forgotten Script is based on secondary sources and full of small errors).

I agree to Renfrew's statement on pp. 3—5: "We are now aware that major developments in human history, such as the emergence of early urban society in the East Mediterranean, were the products of interplay of social and economic factors, and are not usually explained adequately simply by documenting the migrations of groups of people... Archaeology has moved from its preoccupation with races, ethnic groups and prehistoric migration. It has learned to speak with greater authority and accuracy about the ecology of past societies, their technology, their economic basis, and their social organization". The implication is that historical linguistics should move in the same direction, to which I fully agree.

But, as I have already pointed out, I do quarrel with what Renfrew has to say about linguistics. I do think that his criticism of what he terms linguistic paleontology (a term which we no longer use) is unjustified. He ascribes too much weight to the criticisms of Trubetskoy and Bloomfield: both were great linguists who were in the act of creating completely new approaches in the linguistic science, but comparative linguistics has survived both structuralism and descriptionism, after having absorbed their viable ideas. It is "legitimate to reconstruct a Proto-Indo-European language, drawing upon the cognate forms of the words in the various Indo-European languages that are known". The credibility of the result is a matter of correct techniques. If we have Latin *mare*, German *Meer*, Slavonic *more*, "sea", this does not

allow us to reconstruct a Proto-Indo-European **mare*, because the word may have a purely Western Indo-European, and hence probably a substratum origin; one can envisage that Proto-Italic, Proto-Germanic and Proto-Slavonic have been in contact, and had a partially common substratum. But Latin *rex*, Irish *ri* and Sanscrit *rāja* do point to a Proto-Indo-European **reg-*, because no late contacts can here be envisaged. It is wrong to say that PIE knew no gold or silver because there are no common IE words for 'gold' and 'silver': reconstruction from later languages cannot establish what did not exist in the proto-language. But it is correct to say that there was a Common IE word for 'ore' on the base of Latin *aes*, Engl *ār*, Sanscrit *ayas*. The same is true of Lat. *rota*, German *Rad*, and Indo-Iranian *ratha* 'wheel, car' — the languages are too far apart, and a coincidence — given the regularity of the phonetic correspondence — is improbable. Renfrew criticizes the idea of ecological placement of the IE 'homeland' by the terms for trees and animals. He writes (p. 81) "... if we imagine that there was an original homeland outside the boundary [for a particular species of tree on a vegetation map], and that the territories within it came to speak IE languages through the processes of linguistic displacement, then they would need to develop an appropriate vocabulary after their arrival". They might well draw upon words pre-existing the date of the borrowing. Thus, to take one of Renfrew's own examples, Latin *caseus* 'cheese' is not a Common IE but a borrowed word, *viz.*, borrowed after the moment when the change of **-s- > -r-* in Latin became no longer productive. Moreover, it is not only vocables which attest to the affinity or otherwise of the individual languages, but also morphology. Thus several IE languages can be classed as Eastern IE not only on the base of many common lexical isoglosses, but also on the base of grammatical and phonetic isoglosses: the aoristic augment *e-* (Greek, Phrygian, Armenian, Indo-Iranian), the sigmatic Aorist (Greek, Phrygian, Armenian (?), Indo-Iranian, Slavonic), the sigmatic Future or Desiderative (Greek, Phrygian, Armenian (?), Indo-Iranian, Baltic), the satemization of PIE **k'* (Phrygian in some positions, Thracian (>*t*), Armenian, Indo-Iranian, Balto-Slavic practically in all positions). As Renfrew rightly points out, the individual isoglosses do not quite coincide, but it is evident that Greek belongs to the Eastern, not to the Western IE languages. This alone invalidates Renfrew's "Hypothesis A" on the

origin of Indo-Iranian languages, not to count at least a dozen of other weighty counterarguments. According to this "Hypothesis A", there never were any contacts between Proto-Greek and Proto-Indo-Iranian — but how could the sigmatic Aorist and the aoristic augment evolve independently in two separate languages when the habitats of their speakers were thousands of miles apart? Not to speak of the numerous common vocables, noted already by Sir William Jones. Renfrew seems to think that Greek is more close to Italic, which is wrong. I do agree with Krahe (quoted on p. 162; and also with V. V. Ivanov and T. V. Gamkrelidze) that apart from the PIE there have also been intermediate proto-languages. Eastern IE is one of such; however I don't feel that the Western IE are as closely knit together as the Eastern IE are, and anyway, Greek does not belong to the former.

Renfrew's table XI of the Indo-European family is misleading, as are also some other of his tables (thus, Old Church Slavonic is not the ancestor of all Slav languages, but is simply Old Bulgarian; there also exists a slightly russified version of Church Slavonic used by the Russian Orthodox Church; Scythian (Iranian) influence on Slavonic is virtually non-existent; Norwegian and Icelandic derive from Old Norse and not from Danish, to name but a few of the minor errors, which do abound. His tentative identification of the sundry Western IE languages with the different archaeological cultures will certainly need revision.

Renfrew's criticism of glottochronology as introduced by M. Swadesh is justified; of course there is no absolutely constant rate of losses in the core vocabulary. However, the periods of quicker change are usually also the critical but short-lived periods of historical cataclysms etc., and in the long run the process has a more or less constant rate, so that the figures found by Swadesh's method are apt to be correct at least to the order of magnitude when checked by independent data. They also show the 'taxonomic distance', as correctly pointed out by Renfrew. Moreover, a new and more exact technique of glottochronology, involving changes in morphology, has now been evolved by S. A. Starostin in Moscow; Starostin's results relate to Swadesh's approximately as the calibrated radio-carbon dates relate to the non-calibrated ones. It is also clear that loan-words cannot distort the glottochronological picture, because they are usually easy to eliminate.

An amusing case of misplaced criticism is Renfrew's extensive quotation from Ernst Pulgram (p. 85) making a supposed caricature of linguistic reconstruction by 'reconstructing' Latin from the Romance languages. Pulgram thought his caricature was a '*reductio ad absurdum*' of the comparative historical linguistics, and also Renfrew concludes that Pulgram's 'caricature' is a 'marvellous piece of nonsense'. Actually, it is not. It is a very exact and credible reconstruction of the mother language of the Romance speech, namely *Vulgar Latin*. The crass difference between literary Latin and spoken Latin has been brilliantly shown already a hundred years ago by the Polish-Russian linguist Baudouin de Courtenay who used the material of Latin funerary inscriptions.

Renfrew believes in the possibility of linguistic convergence, when two distantly related or unrelated languages become related or more closely related. He even seems to believe that a completely new language can emerge by convergence, pointing out as an example of such "new languages" the pidgins, or creole languages. However, even Tok-Pisin, or Neo-Melanesian Pidgin, is not a 'new' language: it is still a Germanic Indo-European language.

Two typical cases of the "élite dominance" influencing a language are, of course, English with its enormous amount of Old French borrowings, quite outnumbering the Germanic core vocabulary; and Armenian, where Parthian borrowings show exactly the same picture. However, English is still a Germanic IE language, and Armenian is Armenian and not Iranian as Parthian was; this is shown by the preservation of the core vocabulary and of morphological material. And if Armenian has replaced Hurro-Urartian which can be seen from the linguistic substratum material, it was not because there ever existed an Armenian "élite dominance". Although I nearly completely agree with Renfrew's brilliant reconstruction of the models of population and language movements (pp. 124—144), I do think that he overestimates the importance of the "élite dominance" model. An "élite dominance" is feasible only at the level of urban (class) civilization or, at least, in "chiefdom" cultures:

¹ Of course, we will have to eliminate *tabac* and *café*, not only because we know from independent sources that they are late, but also because they do not correspond to the phonetic changes which obtained in and after *Vulgar Latin*. A *Vulgar Latin café* would produce *chef* in French.

this is too late to be relevant for the archaic epoch with which we are concerned.

And now we turn to the main conclusion of Colin Renfrew's book: that the Proto-IE 'homeland' was Anatolia. He thinks that the IE speakers had been farmers from the start, a premise to which I fully agree. From this he infers that their original homeland is the primary zone of agriculture which stretched, about 10,000 years ago, along the inner hill-slopes of the Fertile Crescent. This primarily zone had several 'lobes', viz. the Levantine, the Taurus, and the Zagros 'lobe'. Additionally, there was a primary agricultural zone in SE Turkmenia and, possibly, in the Caucasus and in the Indus valley. Also, for einkorn wheat, there was a primary zone in Western Anatolia.

I fully agree with the idea that the spread of Indo-European languages was connected with the spread of agriculture, and that European — primarily Grecian — agriculture (einkorn, emmer, peas, vetch) had its origin in Western Anatolia, from where it was carried overseas to Crete and continental Greece. — Nevertheless, I am not sure that the first agriculturists already spoke Indo-European, and still less that they spoke Greek. And that is why — as noted by Renfrew on p. 304 n. 10, — I place the 'centre of the Common I-E. area in the fifth millennium B.C.' in the Balkans. There are several reasons why I have selected this solution.

Firstly, what is the Proto-Indo-European language? Actually, unless a linguist works with fixed (although also changing) written languages, he has always to do not actually with languages but with living dialect continuums, dialects, sub-dialects, and socio-linguistic units without sharp boundaries; with bunches of isoglosses etc. The so-called proto-language is a conventional term for a certain dialectal continuum, and the reconstructed proto-forms are always somewhat indeterminate and vague as to the date and space of their distribution in the continuum. This does not mean that the reconstructions are not to be relied upon. They can be likened to the scattering of data points on a graph, which does not hinder the experimenter to draw a mathematically ideal curve. We call a dialectal continuum a 'proto-language' when any two (or more) of its dialects completely lose contact. Therefore, in order to get data on the social and ecological environment of the speakers of a proto-language, it is important not only to know what terms existed in the unbroken proto-language, but also which

terms were inherited by the oldest dialects losing contact. It is a fact that none of the proto-languages of the branches dispersing from the Common PIE can be dated by comparative historical linguistics (and by glottochronology accepted as correct at least to the order of magnitude) to a date earlier than ca. 4000 B. C. This means that Common Indo-European (the proto-language in the sense as defined above) cannot be dated earlier than that (cf. the *rota* evidence). Of course, some ancestor dialects did exist also at earlier periods, and a certain idea of them can be got at by so-called 'internal reconstruction'; but such dialects do not qualify as 'Proto-Indo-European'.

Secondly, as has been shown by S. Illich-Svitych and his friends, the I-E. linguistic family in not an isolated linguistic entity but is a member of a broader linguistic 'order' called Nostratic, or Boreal. Nostratic (to my mind) certainly included, apart from I-E, also Kartvelian (Southern Caucasian), Finno-Ugrian, Uralo-Altai and possibly Elamo-Dravidian; also perhaps Paleo-Asiatic, Na-Dene and Afrasian (Afro-Asiatic). Therefore, hypothesizing about PIE, we must keep in mind that its ancestor dialect originally had contacts with the other Nostratic linguistic families, and specifically with Kartvelian which seems to be especially close to PIE. Therefore I think it more advisable to regard the hypothetical language of the first farmers in Western Anatolia not as PIE proper, but as another proto-language intermediate between PIE and Nostratic.

Thirdly, I do not believe that the first farmers sailing from Western Anatolia to the European continent, actually spoke Greek. There are at least two arguments against this supposition. In Crete, the Greek Linear B inscriptions are later than the apparently non-Greek and quite possibly non-Indo-European Linear A. On the mainland, the Greek language itself, including its earliest known, Mycenaen forms, has a considerable non-Greek and possibly non-IE substratum; the word for 'sea' itself, the element that carried the first farmers from Western Anatolia to Europe, is in Greek a substratum word, *thálassa*. I think that not only IE speaking farmers were the descendants of the farmers of Çatal-Hüyük but probably also the ancestors of the Megalithic culture people (who, to me, are non-IE).

I do not believe that the Hittito-Luwians, or Ancient Anatolians (let alone the Armenians), are the direct descendants of the farmers of Çatal-Hüyük; I believe the latter culture

was entirely destroyed by drought which was the reason of the emigration to Europe about 6000 B.C. It is not true that it is unknown which of the two languages is older, Hattic or Hittite; because there is a number of Hattic substratum words in Hittite but no Hittite or other IE substratum words in Hattic (the example suggested by V. V. Ivanov and T. V. Gamkrelidze does not convince me). I do believe that the Hittito-Luwians, through an 'advance wave' movement, returned to Anatolia from the Balkans (there seems even to have existed certain traits of Urnfield influence (?) on the Luwians), just as the Greeks moved into continental Greece at a date much later than 6000 B.C. The immigration of Phrygians (and, in my opinion, also the Armenians) as well as Thracians, into Anatolia from the Balkans is attested by historical evidence, and there is no reason why the Hittito-Luwians could not precede them in coming the same way. It is true that Hittito-Luwian must have separated from PIE at an early date, because it, like Tocharian, does not belong to Eastern Indo-European, but perhaps to Western Indo-European.

Leading the ancestors of the speakers of IE from the primary agricultural zone, Renfrew claims for them not only the western 'lobe' (from the Taurus to Western Anatolia), but also the eastern 'lobe' (the Zagros); according to his "Hypothesis A", the Indo-Iranians, originating from this zone, later hid themselves somehow from the Sumerians and Akkadians who knew nothing about them, although they did trade with far-away countries east of the Zagros. However, one must consider, that by the same reasoning according to which the European farmers should be traced to the first farmers of the primary agricultural zones, this is obviously true also of the other ancient farmers, such as the Sumerians and Akkadians, the Elamo-Dravidians², the Northern Caucasians (including the Hurro-Urartians), and the Semites; they, as shown by Pelio Fronzaroli who used comparative linguistic methods, — were also originally farmers; and, with the Semites, probably also all the other speakers of Afrasian (Afro-Asiatic) languages should be led from these primary zones. In the case of the Afrasian speakers this conclusion has already been drawn by A. Yu. Militarev who leads them from the Levantine 'lobe'.

It is clear that one should preferably stick to Renfrew's "Hypothesis B" which leads the speakers of Indo-Iranian from the Yamnaya (Kurgan) culture in Russia. A few things ought especially to be noted: the fact that not only are the Avestan and Vedic languages extremely closely related, but this relationship extends even to the identity of the legal terms, which shows that the society of the Common Indo-Iranian speakers was a 'ranked' and probably an urban (class) society; secondly, I should like to draw the author's attention to the very interesting works of the archaeologist E. E. Kuz'min³ who has shown the closeness of the inventory of the Timber-Grave and Andronovo cultures to the description of objects in the Avesta and the Rigveda. Thirdly, note that although, as Renfrew remarks, Brahma and Vishnu, two of the three most important deities of later Hinduism, are mentioned in the Vedas, the third one, Shiva, is not; but it is precisely Shiva who is represented on Harappan objects, along with the sacred fig tree, *Ficus religiosa*, which bears in Sanscrit the substratum name *pippala*.

Thus I would borrow Renfrew's processual approach to ancient population (and language) movements, but I should stick to ca. 5—4000 B.C. for the date of PIE, and regard the farmers of Çatal-Hüyük as Pre-Proto-Indo-European speaking, i. e., as speaking a language which could be the ancestor both of PIE and other languages. And I would certainly leave alone Eastern Anatolia as a candidate for the IE 'homeland'. Eastern Anatolia — or, as we in our country usually term it, the Armenian Highland (exclusive of the southern slopes of the Taurus which belong to the primary agriculture zone), is a land of isolated valleys with, in antiquity, densely forested mountain slopes, unfavourable for very early agriculture, and, at all times, unfavourable for population movements (cf. P. Zimansky's recent book). There are no signs of ancient IE speakers here, and no horses before the second millennium B.C. Thus, the country (just as neighbouring Iranian Azerbaijan) is unsuited for an Indo-Iranian homeland.

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² Renfrew does not have anything to say concerning D. McAlpin's interesting work on Elamite, Brahui, and Dravidian.